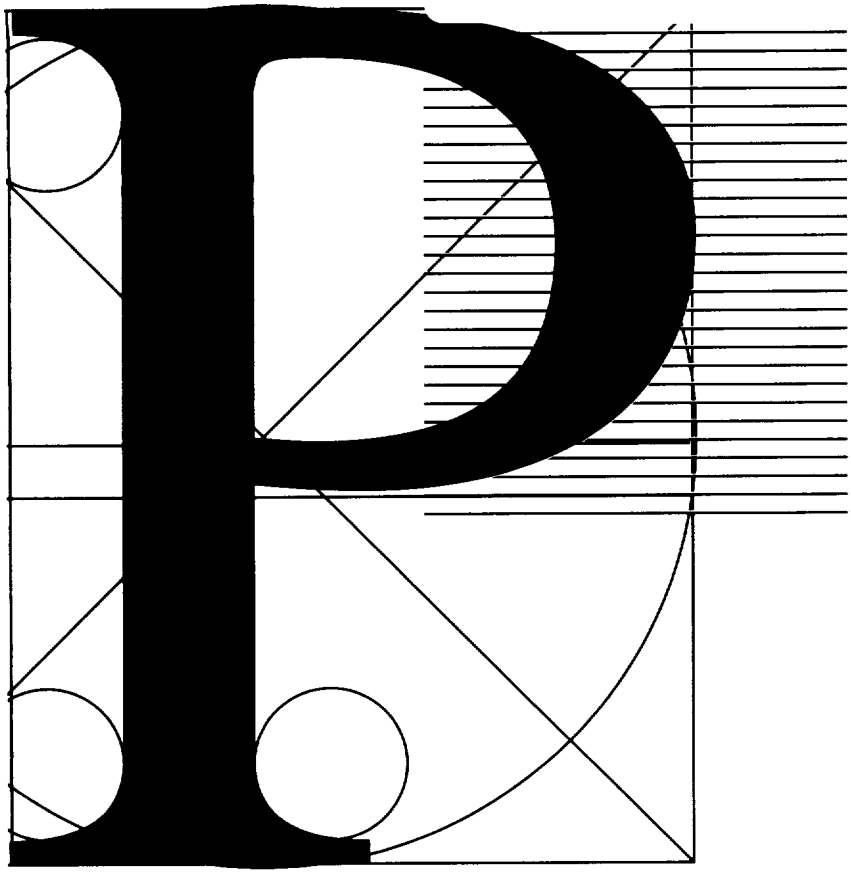

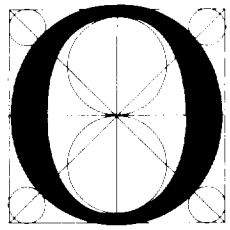

Facilitation and Measurement of Research Publication Productivity

FACILITATION AND MEASUREMENT OF RESEARCH PUBLICATION PRODUCTIVITY

Robert Welch, Leland Stone, and John Greenleaf
NASA Ames Research Center
Life Science Division

The primary goal of the Life Science gravitational biomedical research program at NASA Ames Research Center (ARC) is to investigate the effects of altered gravitational–inertial forces on living systems. To accomplish this goal it is necessary to:

NASA-ARC must continually strive to become the world's leading expert in gravitational biological research.

- Ask pertinent and fundamental questions
- Formulate relevant and testable hypotheses
- Design and conduct research studies to address these hypotheses
- Publish research results in the most prestigious, peer-reviewed journals or books.

ARC must continually strive to become the world's leading expert in gravitational biological research. To meet this aspiration it is not enough merely to do good and exciting research; this research must be disseminated to other scientists and to the community at large by means of publication in research journals, books, and other written media. The aim of this document is to evaluate the process of scientific publication as it pertains both to the Life Science Division of ARC and to the overall NASA research effort. Specifically, we will address the issues of:

- Why scientists should publish their research findings
- Why NASA management has a vested interest in this endeavor
- How managers can facilitate this activity
- How publication performance can be measured and evaluated.

To publish is "to declare publicly; to make generally known." Thus, publications can take a variety of forms, including journal articles, books, abstracts, and technical memoranda. Even oral presentations at conventions and conferences qualify according to the definition above, although our emphasis here will be on written forms of communication. Publishing research results is an essential aspect of being a scientist. Failing to do so retards the advancement of scientific knowledge. Scientific achievements are rarely created in a vacuum from a sudden and dramatic insight, but are built upon accumulated published data. Furthermore, advances in technology and in human health and well-being are greatly facilitated by the easy availability of published research findings.

Scientific achievements are rarely created in a vacuum from a sudden and dramatic insight, but are built upon accumulated published data.

There are many reasons why scientists publish:

- **Peer review** of scientific manuscripts provides authors with feedback that usually improves their logic, clarity of presentation, and conclusions. Frequently, peer review also raises questions for further research.
- **Dissemination of information** among researchers, and between them and the lay community, replaces hearsay and counteracts the claims of charlatans.
- **Research grant** support is often based on published research because it documents research progress.
- **Priority and proprietorship** of an idea or invention are determined when research is published. This contributes to the prestige of authors and their institution and, in some cases, facilitates the determination of patent rights.
- **Inadvertent duplication of research** studies or reinvention of procedures or techniques is prevented by timely publication of research findings, thereby saving time, money, facilities, and resources, and minimizing use of laboratory animals or test subjects.
- **Promotion** of research scientists who publish is largely based on their publication record because this record is the most objective yardstick available for evaluating their scientific achievements.

- **Clarification and extension** of scientific studies occur when other investigators discuss, criticize, replicate, and otherwise respond to published findings.
- **Scientists have a duty** to their employer (i.e., institution, agency) and to their customers (foundation, tax payers) to publish their research results.
- **Scientists enjoy recognition and receive personal satisfaction** by advancing their field of study through the contribution of original, published knowledge.

Performance of high-quality research and preparation of reports for publication requires a supportive management philosophy for allocating adequate time, facilities, support personnel, and funding. There are several reasons why it is in the best interest of NASA management to encourage and facilitate research and publishing.

Performance of high-quality research and preparation of reports for publication requires a supportive management philosophy for allocating adequate time, facilities, support personnel, and funding.

First, because NASA research proposals are reviewed primarily by members of the academic community, to be successful in obtaining research funds, NASA scientists must establish and maintain a record of publishing important, peer-reviewed scientific work in appropriate, high quality, and widely read journals.

Second, the publication of high quality papers attracts other scientists and postdoctoral fellows to ARC—some of them even bring their own funds and collaborators. Scientists generally travel to other laboratories to collaborate with fellow scientists, rather than simply to work with their specialized equipment.

Third, ARC acquires greater prestige whenever "NASA Ames Research Center" appears in print as the sponsoring organization for important research.

Fourth, an extensive, quality publication record suggests to supervisors (and research grant review committees) that the scientist in question is capable of conducting and completing research projects.

Fifth, because supervisors are not always expert in the scientific fields of the scientists they must evaluate, an extensive and continuous record of publishing in *peer-reviewed* journals will provide clear evidence that other scientists who *are* in a position to evaluate this person's accomplishments have found them to be noteworthy.

The following proposals are offered as a means of facilitating the research and publication records of research scientists in the ARC Life Science Division.

It is in the best interest of NASA management to encourage and facilitate research and publishing.

- **Scientists should be funded to attend one meeting each year without submitting an abstract.** The current NASA policy that scientists must present a paper or a poster at a meeting in order to receive travel funds encourages submission of sloppy, partial, or premature work that may not qualify for publication. Allocation of funds to travel to subsequent meetings in a given year should continue to require a presentation or other significant contribution (e.g., chairing a session).
- **The ratio of published abstracts to published, peer-reviewed journal articles should be reduced.** Ideally, this ratio should be about 1:1.
- **For each article submitted for publication, managers should urge authors to publish their data in an appropriate NASA publication as well (e.g., a Technical Memorandum), especially if practical findings are presented.** The primary reason for this proposal is that project managers are more likely to read NASA publications than journal articles. Furthermore, the NASA document is a more appropriate vehicle for discussing practical applications of research findings for problems in the aerospace program. Finally, it is an appropriate place for inclusion of extensive raw data, tables, and text that are usually too detailed for journal publication. Thus, the material in the NASA publication should *not* be an exact duplicate of the journal article.
- **A more rigorous procedure should be implemented for in-house review of manuscripts submitted for publication as NASA documents.** For example, two reviewers with the appropriate expertise could be selected (perhaps from names suggested by the senior author) from NASA, contractor, or academic personnel. Their reviews would be sent to our Deputy Division Chief, who would act as editor. In addition, management should continue to support and improve our good relationship with the Publications Branch to

expedite processing of journal articles, technical memoranda, and other publications.

- **A formal and efficient process should be established for transferring our publications to other NASA Centers and NASA Headquarters.** Publication in the general scientific literature is not always the best conduit for transmitting information to other NASA Centers. Intra-NASA information transfer may be more efficient via NASA publications that are routinely sent to other NASA centers. This will reduce the time for the realization of practical applications of research findings.

Outstanding, well-known scientists generally publish their findings in high quality journals. Sometimes, however, publishing in a less prestigious journal may be the best means of reaching the desired audience, thereby making it the more appropriate outlet. Thus, publishing can take a variety of forms for different purposes.

Measuring the quantity of publications is easy; evaluating the quality is much more difficult.

Measuring the quantity of publications is easy; evaluating the quality is much more difficult. Two major criteria for evaluating quality are the scientific standing of the editor and editorial board members of the journal and the percent of submitted manuscripts rejected by the editor. The relative importance and prestige of various publications may be debated. However, one **possible** ranking, beginning with the most valued, follows:

- Sole author of a peer-reviewed investigative or review article in a prestigious journal
- First author of an article in a prestigious journal
- Senior or co-author of an article in a prestigious journal or of a book
- First author of an article in a lesser quality, peer-reviewed journal
- Editor of a book or conference proceedings
- Senior author (who may be the last author) of an article in a prestigious journal (ref. 1)
- Patentee
- Co-author of an article in a lesser quality, peer-reviewed journal
- Author of an invited chapter
- Author of a special publication or other NASA publication
- Author of a published abstract, book review, or published address.

Another important criterion is authorship of a publication that receives many citations by other research scientists (ref. 2).

Not listed is the funded grant proposal (e.g., from NASA, NIH, NSF). Although a proposal is not generally considered a publication because of its restricted access, it has some of the characteristics of a publication such as being a relatively permanent, peer-reviewed

document. It is not clear how a funded proposal should rank relative to the other more traditional publications, but it should feature in the scientist's job performance evaluation.

The order of authorship for a published document is a delicate matter and should be determined by the research team (ref. 1).

A research scientist's publication productivity could be measured in terms of a *publication efficiency index (PEI)*:

$$\text{PEI} = \frac{(\text{No. of Pub. or No. of Pub. Pages}) \times \text{Pub. Quality}}{(\text{Grant Dollars/2 Years}) \times \text{Available Research Time}} = \text{Pub./Cost}$$

where:

Research findings should be reported adequately and properly whether it requires one or 10 publications.

Number of Publications or Number of Published Pages is self-explanatory.

Publication Quality is a numerical weighting based on prestige ranking.

Grant Dollars/2 Years is the average funding available over the preceding two years (because current publications are usually based on prior funding).

Available Research Time is time actually allocated to conduct research (after subtracting time required to carry out administrative duties, mission projects, or other non-science duties).

Using Grant Dollars/2 Years in the denominator of the PEI makes it an efficiency index (output per unit input). The greater the PEI ratio, the more the researcher is producing relative to funding. Thus, a higher PEI should be rewarded. Importation of research funds is a poor measure of research performance without simultaneous evaluation of output. No researcher's job performance evaluation should use the PEI calculated for a single year because of year-to-year PEI fluctuations; a cumulative average over three to five years would be more appropriate and fair. Finally, the PEI proposed here is only one of many possible quantitative indicators of a publication record.

Clearly, some areas of research lend themselves to more opportunities for publication than others. For example, a single longer publication (one that reports on several experiments) may be as or more important than several shorter publications, each reporting on a single experiment. In any case, research findings should be reported adequately and properly whether it requires one or 10 publications.

Conclusions

Exciting research is going on in the Life Science Division of ARC, and it is imperative that results of this research reach the widest possible audience. It is our hope that this document will facilitate that process. The publication record of NASA scientists is the fairest and best measure of their contribution for achieving the NASA vision.

Acknowledgments

The authors would like to thank the people who helped make this document possible. Bethann Dennis, Technical Editor, who reviewed the numerous versions of this document; Dave Faust, who designed the document; and Steve McKnight, Art Director, who organized the layout and paste-up of the document.

References

1. Riesenberg, D. and Lundberg, G. D. "The Order of Authorship: Who's on First?" *J.A.M.A.* 264 (1990): 1857.
2. Siekevitz, P. "Citations and the Tenor of the Times." *FASEB J.* 5 (1991): 139.

THE ORDER OF AUTHORSHIP: WHO'S ON FIRST?

From the Journal of the American Medical Association, 1990.

The authorship of medical articles is under scrutiny as never before. Fraudulent work appears, then may enjoy citation in the literature long after retraction. In academia, most appointment and promotion committees tally the number of publications of a candidate, a recent proposal for some limit notwithstanding. Names of prominent, senior scientists appear in bylines, so called honorary authorship, as a means of impressing editors and reviewers and to acknowledge moral or financial support.

Perhaps the responsibilities of authorship come into clearest focus when investigators decide on the order in which their names will be listed on their manuscript. The designation of first author and the sequence of listing are important for several reasons. Some landmark studies are known by the name of their first author, lending support to the impression that, being listed first, he or she played a pivotal role in performing the work and writing the article. By tradition, many reserve the position of last author for the senior member of the research team or the department or division director, but this encourages honorary authorship (see below). First-listed author vs, say, sixth on a major article can carry substantial weight in the attainment of those academic rewards to which investigators rightly aspire. Finally, readers deserve articles that are clear in every respect, including a sense of the relative contribution of each author.

How then do coworkers decide among themselves on the order of authorship? The task is made difficult not only by human nature, which dictates that there be bias and jealousy, but by the complexity of scientific research: does the biochemist or the epidemiologist deserve more credit for this work?

There is no shortage of suggested solutions in the literature. These have included mathematical formulas; multiple categories of authorship, denoted by "coded credits"; and a worksheet assigning weight to various aspects of the investigation. The oft-heard call for alphabetical listing of authors is a cop-out and takes none of the above subtleties into account. The International Committee of Medical Journal Editors has grappled with this problem for some years but has yet to recommend a solution.

We believe, based on discussions with many persons with academic experience and editorial expertise, including the *JAMA* Editorial Board and staff (among whom consensus has not been achieved), that guidelines for determining the order of authorship could be straightforward:

- It is first essential to decide who is an author and who is not. Requirements include participation in the work and the writing, assumption of public responsibility for the conclusions, and willingness to submit the data on which the study is based if so requested.
- Being a laboratory or departmental sponsor and (last) author are not mutually exclusive, but should depend on contribution to the work being reported. However, the awarding of honorary authorship is intellectually dishonest, inflates bibliographies, and dilutes credit for scientific work: the practice is unacceptable. Persons who provide support and advice not consonant with authorship may, with their consent, be thanked in an acknowledgment.
- The first author is that person who contributed most to the work, including writing of the manuscript (an author is a person who writes). The sequence of author listing is determined by the relative contributions to the work.
- Decisions about authors and the order in which their names will appear should be made as early as possible, even at the outset, although relative contributions may need to be reassessed later by group consensus.
- Disagreements about these matters should be resolved by the principals, not by the editor. However, editors can request documentation in writing of authors' specific contributions to the project.

Journal editors care about accuracy and truth but are only advisers to authors, the overwhelming majority of whom are right-minded, dedicated, and very busy. Surely most misjudgments about authorship result from failure to ponder its meaning.

We welcome comments responding to these ideas.

Don Riesenberg, MD
George D. Lundberg, MD

CITATIONS AND THE TENOR OF THE TIMES

From the FASEB Journal, 1991.

Recently, Science magazine requested from the Institute of Scientific Information (ISI) data as to the number of times papers are cited in the scientific literature. The calculators at the ISI came up with a figure that showed 55% of the papers published between 1981 and 1985 in journals examined by the ISI were not cited at all in the 5 years after their publications. Various leaders of science expressed amazement at that large percentage. Some raised the obvious question as to whether much of the research being done is worthwhile at all.

Good question, for the extent of and the growth in the scientific literature in the last two decades has amazed, confounded, and beset researchers. However (and it is a large however), no one, as far as I know, has questioned the validity of the data that the ISI puts out with much fanfare. Bibliographies and reference lists are compiled by the authors themselves, with the foibles that surmount and confound we mere humans, with the feverish haste with which we scientists are all familiar, and with the lackadaisical effort that all too frequently entails the completion of a paper. So the question arises as to the worth of the reference lists used by the ISI. Are they the result of conscientious effort or are they the scans of the scientific literature? Are they the result of pre-experimental effort and/or the result of post-experimental stages? Are they the result of extensive researches of the literature at any stage of the research project, or are they the result of perusals, even hasty perusals, of selected journals from selected countries of origin? Until the ISI answers these questions, I will cast a quizzical eye on any calculations coming out of their mathematics. Thus, I would discard all the data emanating from the ISI's citation indices and the conclusions they draw from them. To me, it is rather surprising that scientists, who are expected to question suppositions, take the citation indices at face value and have not, as far as I know, examined their basis in fact.

Let us look at ourselves. We set out to do a series of experiments that are undoubtedly based on our own work. This creates the high degree of self-citation. But how many of us faced with a new problem assiduously search the literature, when other laboratories may have already tackled the problem or a segment of the problem? How many of us will find that a solution to the problem, partial or complete, may be found in a paper on a related subject? I would guess that this searching is not done too

often, and that if it is, it is perfunctory, that much of our current individual work is based on our own findings and not on the results of others. Some administrators of science have asked whether much of the research being funded is worthwhile, since it seemingly does not get cited, it is unknown, and therefore does not contribute to furthering scientific research. I would raise the opposite question: Is much of ongoing research (even that done by acknowledged leaders in the field) redundant? Has it been conducted and published by authors unknown, in journals unknown? If the results in these papers were known, then some of the present work has become unnecessary, a waste of time, effort, and money. I would guess that out there in the forgotten literature are helpful methodologies that have been overlooked, that out there there exist many seemingly minor points in related papers, which, if known, could shorten the duration of experiments being done at present, that past results overlooked could prevent errors in the present. Thus, the gauntlet should be thrown down, not upon the hundreds of papers never cited, but upon those who refuse to acknowledge or cite them, deliberately or not, who refuse to admit that out there are possible gems if only they would take the time to dig them out.

What does this say about the tenor of our times? The burgeoning of the scientific literature, the difficulty of keeping up with it all, is an excuse by many scientists to discard much of it as meaningless and to reserve a small proportion of papers to a category of "real" science. Thus, they cite each other, giving rise to breakthroughs, seeking out the mass media, quoting each other for the benefit of the public, issuing gratifying euconiums to their own superiority. They seize on the ISI reports as proof of their contentions, accept as scientific truth what is really scientific greed, and virtually read out of science those whom they have ignored. But I am sure that in the vast extent of the literature there is much out there which has been, and is, disregarded, as not worthy of even being sought out, perhaps because it is published in journals not of the self-acclaimed first rank, from laboratories little known, from countries ill-regarded. There is a certain parochialism in all this, because to many, there is "western" science and then there is all else.

But a rapid change is taking place in what may be called a "democratization" of the scientific endeavor. There is nothing arcane about the methodology of western science with its standards of thought and philosophy, which cannot be learned in laboratories all around the world. Passing are the days when a very small group of scientists set the

pace, and a much larger group is busying itself filling in what has been called the inconsequential steps.

Scientists, being human, justify themselves and their work: they compete strongly among themselves, sometimes they demean others in the pursuit of their own aggrandizement, they tend to cite others grudgingly to notch up their self-worth, and they end up doing a disservice to themselves and to the scientific endeavor.

Lately, to add to this competitive momentum, has been the pursuit of money. Some wealth is to be gained in tying into commercial interests, in setting up companies, and in pursuing this elusive rainbow. Pats on the back have become a necessity. Thus, the work of others is ignored, and rationales are devised for this hiatus in the literature.

The intense competition for recognition by peers and by the general public, for prizes, for commercial gain, is slowly eroding the scientific ethic. This is the ethic that depends on cooperation among scientists, on a morality that drives out selfishness, on the acknowledgments of and by others. And if this ethos is disappearing, then the citation indices no longer reflect worth but a lack of the scientific communitas. The future of the scientific endeavor depends on regaining the scientific soul.

Philip Siekevitz
Professor Emeritus
The Rockefeller University